REPRINTED FROM THE TRANSACTIONS OF THE
CONNECTICUT ACADEMY OF ARTS AND SCIENCES

INCORPORATED A. D. 1799

VOLUME XIII. PP. 1–46 MAY, 1907

Publications of Yale University

THE EFFECT OF DIET ON ENDURANCE

BASED ON AN EXPERIMENT, IN THOROUGH MASTICATION, WITH NINE HEALTHY STUDENTS AT YALE UNIVERSITY, JANUARY TO JUNE, 1906

BY

IRVING FISHER, Ph.D.

PROFESSOR OF POLITICAL ECONOMY AT YALE UNIVERSITY

NEW HAVEN, CONNECTICUT
1907

THE TUTTLE, MOREHOUSE & TAYLOR PRESS

I.—The Effect of Diet on Endurance, Based on an Experiment with Nine Healthy Students at Yale University. January–June, 1906.

Introduction.

There appears to be very little literature on the subject of endurance. Since the epoch-making work of Mosso, much has been written on fatigue, and many varieties of ergographs have been constructed to record muscular fatigue; but no systematic study of endurance as such appears to have been made. Even the concept of endurance, as related to strength and fatigue, has been lacking. No correlations have been worked out between endurance and the factors upon which it depends, except that it has been a matter of common experience that endurance increases with exercise. In respect to diet, opinions as to its relation to endurance, so far as the writer knows, have rested on no better foundation than the personal impressions of adherents of special dietary systems, such as those of Salisbury, Dewey, Haig, Kellogg, and Fletcher. In Professor Chittenden's painstaking study on "Physiological Economy in Nutrition" he has shown that one result of a gradual and systematic reduction in proteid, from the amount ordinarily consumed, has been an increase in *strength*, but no data were obtained in respect to *endurance*.

The present experiment had a somewhat accidental origin. I was engaged in collecting statistics of labor-power in relation to various factors, among them especially diet. The data were collected because of their economic bearings and without any intention at first of making independent experiments. But some of my students, whom I had engaged to make computations and diagrams, became interested in the material with which they thus came in contact, and expressed a strong desire to try dietetic experiments upon themselves. Not being a physiologist, I asked Professor Chittenden if he could not take charge of these experiments for them. It so happened that on account of other similar work he was unable to do so, but suggested that I should conduct them myself. I have done so with considerable hesitation, not being equipped for physiologic studies. I have therefore restricted my attention to the simpler practical aspects of the problem, although some of the technical points have been inves-

tigated through the very kind coöperation of able colleagues. My thanks are especially due to Professor Chittenden and his co-workers, Professor Mendel and Dr. Underhill, for the aid rendered by the Sheffield Scientific School Laboratory in determining the nitrogen excreted, and for much helpful advice and criticism. I wish also to express my obligations to Dr. J. P. C. Foster for his services as medical adviser to the students; to Dr. W. G. Anderson, Director of the Yale Gymnasium, and his corps of assistants, through whom the endurance tests were conducted; to Professor Rettger for fecal tests; and to the subjects of the experiment themselves, Messrs. Bauer, Edwards, Lagerquist, Lawton, Mitke, Parmelee, Reeds, Taylor, and Weyman, whose patient submission to the painful tests of endurance was little short of heroic.

In January, 1906, the students above mentioned organized themselves into an eating club. The experiment began with an endurance test on January 14, and consisted of two main parts, each of which lasted about ten weeks.

The object of the first half of the experiment was to test the claims which have been made by Mr. Horace Fletcher, as to the effects upon endurance of thorough mastication combined with implicit obedience to appetite. Our conclusion in brief is that Mr. Fletcher's claims, so far as they relate to endurance, are justified.

Mr. Fletcher's method may be briefly[1] expressed in two rules.

1. *Mastication.* Thorough mastication of all food up to the point of involuntary swallowing, with the attention directed, however, not on the mechanical act of chewing, but on the tasting and enjoyment of the food; liquid foods to be sipped and tasted, not drunk down like water. There should be no artificial holding of food in the mouth beyond the time of natural swallowing, even if, as is to be expected at the start, that swallowing is premature. It is not intended to "count the chews," or hold the food forcibly in the front of the mouth, or allow the tongue muscles to become fatigued by any unnatural effort or position, or in any other way to make eating a bore. On the contrary, every such effort distracts one from the natural enjoyment of food. Pawlow has shown that without such attention and enjoyment of the taste of food, the secretion of

[1] The reader who desires to pursue the subject is referred. as to mastication and instinctive eating, to Higgins, *Humaniculture*, Stokes, N. Y., 1906; as to proteid, to Chittenden, *Physiological Economy in Nutrition*, Stokes, 1904; and as to the general subject, to Horace Fletcher, *The A. B.-Z. of our own Nutrition*, Stokes, 1903.

gastric juice is lessened. The point of involuntary swallowing is thus a variable point, gradually coming later and later as the practice of thorough mastication proceeds, until the result is reached that the food remains in the mouth without effort and becomes practically tasteless. Thus the food, so to speak, swallows itself, and the person eats without thought either of swallowing or of not swallowing it; swallowing is put into the same category of physiological functions as breathing, which ordinarily is involuntary.

2. *Following instinct.* Never to eat when not hungry, even if a meal (or more than one, for that matter) is skipped. And when a meal is taken, not to be guided by the quantity of food offered, or by past habit, or by any theories as to the amount of food needed. The natural taste or appetite is alone consulted, and the subject selects, from the food available, only those kinds and amounts which are actually craved by the appetite. After practice, the appetite gradually becomes more definite and discriminating in its indications.

These two rules — thorough mastication and implicit obedience to appetite — were alone employed during the ten weeks which constituted the first half of the experiment.

Shortly after the beginning of the second half of the experiment, there was an interim of six days at Easter recess, during which the few men who remained in New Haven took advantage of the temporary absence of the cook to try the possibilities of living without one entirely. During this brief period use was made not only of raw foods, such as fruits, nuts and milk, but also of foods which could be purchased already cooked, such as the flaked breakfast foods. But all the food was cold, and several of the men found it cheerless and unsatisfactory. Judging from their feelings, they were losing in weight and vigor. This part of the experiment was too brief, however, to justify any reliable conclusion as to the virtues of a raw, or rather a cookless, diet.

The second half of the experiment lasted about nine weeks. The same two rules which were employed during the first half were continued during the second, but a third rule was added. This was the use of suggestion, as follows:

3. *When instinct is in doubt, use reason.*—This rule consists of acquiring and applying a little knowledge of foods and food elements. For this purpose, in the present experiment two lists of food were given. One was arranged in a tentative order of intrinsic merit, beginning with fruits and ending with alcohol, and the other in

the order of the proportion of proteid. The men were then asked, when and only when the appetite was *entirely willing*, to choose the better and purer foods and the low proteid foods in preference to those high in proteid. In this way the men gradually shifted their diet upward in the two lists, and thereby pursued a little faster the same direction in which they had already been found to be unconsciously moving under the influence of thorough mastication and implicit obedience to appetite.

It would too greatly lengthen this report if any attempt were made to repeat in detail all the specific advice given to the experimenters under Rule 3. What has been said covers in a general way all the points except the advice (subject always to the consent of appetite) to eat light and quickly digested suppers in order to go to bed on an empty stomach.

Careful record of the amounts of food eaten and the constituents of proteids, fats and carbohydrates was kept for each man each day, certain days being omitted if for any reason the record was incomplete, as when, for instance, the men were out of town or took their meals away from the club.[1] To avoid weighing at the table, the food was all weighed in the kitchen and served in "standard portions" of 100 calories each, or simple fractions or multiples thereof, and the men merely recorded the number of portions eaten. The proportions of proteids, fats and carbohydrates were found by means of the writer's "Mechanical Diet Indicator."[2] Atwater and Bryant's tables were used as a basis for calculation. For the first few weeks the figures were probably subject to some errors, and in all cases more or less guessing had to be practiced with reference to the amount of lean and fat of meats; but the influence of any errors on the results must necessarily be small, because meat supplied, at the highest, only a small fraction of the total calories. It is believed that the results are in general correct to two significant figures.

For the first two weeks of the first half of the experiment, the men ate in their ordinary way. During the following eight weeks they masticated more thoroughly and followed the leadings of taste more carefully. Most persons, while nominally following taste, are largely

[1] The number of days each week on which the record of diet was kept was seldom under six.

[2] For a description of this instrument, see the writer's "A New Method of Indicating Food Values," *American Journal of Physiology*, April, 1906. For a description of its practical uses see "A Graphic Method in Practical Dietetics," *Jour. of the Amer. Med. Assoc.*, Apr. 20, 1907.

controlled in their selection of foods by many other circumstances,—as, *conventionality*, or the desire to eat what others eat and the unwillingness to appear "different"; *politeness*, the desire to please one's host and hostess; *food notions*, the opinion that certain foods and certain amounts of food are "wholesome" even if not palatable and that certain foods should be avoided as injurious even if delicious to the taste; *narrowness of choice*, as at a boarding house table, which often supplies what is not wanted and withholds what is; and *habit*, by which the particular kinds and amounts of foods which have become customary through the previous causes—conventionality, politeness, food notions, and narrowness of choice—are repeated day after day without thought. The subjects of the present study were given a wide range of choice, the menu including fruits, nuts, cereals, puddings and pastry, vegetables, milk, meats, etc. Meat if desired was available three times a day.

The object of the experiment was to find what effects on diet and endurance would follow from a strict obedience to the taste-instinct, when this instinct was given a longer chance to act by prolonged mastication and attentive tasting. Each man was therefore encouraged to choose his own food out of the menu for the day. Nothing was set before him until it was ordered, and even after a food was ordered it was not eaten if taste did not so dictate. The men were specially warned, during the first half of the experiment, against any conscious effort to decrease their food, proteid, or meat; and while it is possible that subconscious suggestion played a part, so far as could be observed they were freer from its influence than any ordinary experimenter who might take up the same experiment after reading Mr. Fletcher's or Professor Chittenden's books.

That this conclusion as to the relative absence of subconscious suggestion is correct was evidenced by the experiences both before and after this part of the experiment. For a month prior to its actual beginning (Jan. 14), the experiment had been fully decided upon, and its plan and scope understood by the men. Had subconscious suggestion played an important rôle, it would probably have shown itself in a reduction of proteid during this month; but determinations of the grams of nitrogen daily excreted in the urine, taken at the beginning and end of this month, indicated no substantial change, as the following table shows. (M. does not appear in this table, owing to the absence of any specimen for December.)

TABLE I.

	B	E	Lq	Lw	P	R	T	W	Average
Middle Dec.	11.2	11.1	13.8	12.3	11.3	13.9	14.2	15.9	13
Middle Jan.	10.4	12.7	14.3	14.3	11.1	14.8	12.2	15.4	13.1

On the other hand, during the second half of the experiment (Mar. 28–June 1), when the force of suggestion was consciously introduced, the reduction of flesh and proteid went on rapidly, as is seen in Table II. The facts, therefore, seem to show that the men followed directions closely, avoiding largely the influence of subconscious suggestion and following that of conscious suggestion in exact accordance with the directions given them.

Changes in Diet.

During the first two weeks of the first period when no change of habits was undertaken, the food showed little tendency to change in amount or in kinds. On the other hand, for the remaining eight weeks, during which thorough mastication and instinctive eating were practiced, there was a distinct though gradual tendency toward reduction in the amount of food, in the quantity of proteid, in the quantity of flesh foods, and in the quantity of liquids of all kinds—water, tea, coffee, cocoa, and even soups. Exact figures were kept for calories, proteid and flesh foods. These showed that the total calories gradually fell about 10%, the proteids, 15%, and the flesh foods, 40%.

In the second period, during which the force of suggestion to reduce proteid and flesh foods was added, the same effects were noted in a still greater degree. During this period the calories dropped nearly 20%, the proteid over 25%, and the flesh foods about 70%. Comparing the diet at the close of the entire five months of the experiment with the diet at its beginning, it was found that the total calories had fallen about 25%, the proteid about 40%, and the flesh foods over 80%, or to about one-sixth of their original amount. A part of the reduction, at least of the calories, is probably due to the change in season, as the experiment began in cold weather and closed in hot weather.

These results are shown in the following table :

TABLE II.

AVERAGE DIETETIC RECORDS OF ENTIRE CLUB

	Week	Average weight	Average no. of "portions" daily	Calories of proteid per lb. of body wt. [1]	Daily "portions" of flesh foods
1st Period	Jan. 17–23	149.8	28.3	2.7	2.4
	24–30		30.3	2.6	2.1
	31–Feb. 6		27.8	2.5	1.6
	Feb. 7–13		27.6	2.4	1.2
	14–20		25.8	2.1	.9
	21–27		26.4	2.2	1.1
	28–Mar. 6		25.3	2.2	1.2
	Mar. 7–13		24.6	2.1	1.3
	14–20		25.9	2.2	1.1
	21–27	148	26.7	2.2	1.4
2nd Period	28–Apr. 3		26.7	2.1	.8
	Apr. 4–10		25.7	1.9	.5
	11–17 [2]		27.3	1.7	.4
	20–26 [3]		26.1	1.7	.1
	27–May 3		25.5	1.9	.5
	May 4–10		25.7	1.9	.4
	11–17		26.2	1.9	.4
	18–24		24.9	1.7	.4
	25–31		22.2	1.4	.4
	June	144			

Remembering that a "portion" is 100 calories, we see that, during the first four weeks, the men consumed an average of from 2760 to 3030 calories per day, of which 120 to 240 were in the flesh foods, such as meats, poultry, fish and shell-fish, and that 2.4 to 2.7 calories of proteid were ingested for each pound of body-weight. Translating Professor Chittenden's figures for the physiological requirement of ingested proteid, we find it to be from 1.3 to 1.7 calories per pound of body-weight. Thus the men were at this time consuming nearly double the Chittenden allowance. During the last four weeks of the experiment all these magnitudes were lower. The per capita calories ranged from 2220 to 2620, of which only 40 were in flesh foods, and the proteid had fallen to 1.4 to 1.9 calories per pound of body-weight, which corresponds closely to the Chittenden standard.

Table II was constructed from the following three tables giving separate data for the individual experimenters.

---

[1] This column is calculated throughout on the basis of the body-weights on Jan. 14.

[2] Except E., M. and P.

[3] Except E. The last two days of the Easter recess, Apr. 18, 19, are omitted in tables II, III, IV, V.

TABLE III.

DAILY QUANTITIES OF FOOD

(in "portions" of 100 calories each).

	Week	B	E	Lq	Lw	M	P	R	T	W	Average
1st Period	Jan. 19–23	25.4	26.1	22.4	32.6	23.9	27.0	32.0	36.1	29.4	28.3
	24–30	27.9	29.1	24.3	32.6	26.8	26.2	34.5	37.3	33.9	30.3
	31–Feb. 6	26.3	26.4	25.0	29.8	24.4	23.8	30.9	30.8	33.0	27.8
	Feb. 7–13	25.7	27.8	25.7	31.3	22.0	23.2	28.0	32.8	31.6	27.6
	14–20	24.0	32.4	24.0	26.1	21.6	20.3	25.1	29.2	29.4	25.8
	21–27	23.0	30.9	23.4	28.3	24.5	18.2	26.7	30.3	32.2	26.4
	28–Mar. 6	23.3	25.7	23.6	27.4	24.1	18.0	25.4	29.1	30.8	25.3
	Mar. 7–13	23.2	23.0	24.5	27.4	26.4	19.2	23.5	23.7	30.1	24.6
	14–20	21.9	21.6	24.5	29.1	30.2	20.8	25.0	30.8	28.9	25.9
	21–27	21.1	24.2	25.2	31.6	25.9	21.2	26.5	34.4	30.4	26.7
2nd Period	28–Apr. 3	22.6	26.3	26.2	27.5	25.2	23.0	26.1	33.5	30.2	26.7
	Apr. 4–10	24.2	24.2	24.6	27.7	22.5	22.5	26.2	30.5	28.7	25.7
	11–17	24.0	....	26.0	24.9	....	....	29.0	30.4	29.8	27.3[1]
	20–26	26.5	....	24.1	24.5	23.6	23.3	25.5	32.0	29.4	26.1[2]
	27–May 3	25.5	22.6	25.4	24.8	24.1	24.4	23.9	29.0	29.8	25.5
	May 4–10	23.0	25.5	26.5	24.4	27.1	23.1	26.0	28.7	26.7	25.7
	11–17	21.8	19.2	26.5	27.6	24.5	23.8	27.6	30.3	34.2	26.2
	18–24	22.7	19.1	23.7	27.8	19.5	25.4	24.6	26.5	35.2	24.9
	25–31	19.3	17.2	23.6	27.1	17.8	21.9	24.2	20.3	28.6	22.2

From this table we see that there were wide differences between the men in regard to the change in the quantity of food. During the first period the men who reduced their calories conspicuously were B., P. and R., the very men, as Table IX will show, who lost weight during this period.

During the second period, reductions were noticeable in E., Lw., M. and T. These, together with B. and P., were the men who lost weight during the second period. We see here a distinct correlation between quantity of food and loss of body-weight.

TABLE IV.

PROTEID (in calories) PER LB. OF BODY-WEIGHT

(Body-weight as taken Jan. 14, 1906)

	Week	B	E	Lq	Lw	M	P	R	T	W	Average
1st Period	Jan. 17–23	2.6	2.9	1.8	2.8	2.6	2.8	2.5	3.3	2.7	2.7
	24–30	2.5	2.8	2.2	2.8	2.7	2.5	2.5	3.0	2.8	2.6
	31–Feb. 6	2.2	3.1	2.4	2.9	2.5	2.0	2.3	2.7	2.6	2.5
	Feb. 7–13	2.1	2.9	2.2	2.6	2.0	2.1	2.0	2.9	2.6	2.4
	14–20	2.0	3.1	1.8	2.0	1.9	1.7	1.8	2.7	2.2	2.1
	21–27	1.8	3.2	1.9	2.3	2.0	1.5	1.9	2.8	2.4	2.2
	28–Mar. 6	1.8	3.0	1.8	2.3	2.4	1.5	1.8	2.6	2.4	2.2
	Mar. 7–13	1.9	2.5	2.0	2.3	2.5	1.7	1.7	2.1	2.6	2.1
	14–20	1.9	2.3	1.9	2.5	2.7	1.7	1.9	2.8	2.4	2.2
	21–27	1.6	2.6	2.0	2.7	2.4	1.7	1.9	2.7	2.2	2.2

[1] Not including E., M. and P.

[2] " " E.

TABLE IV—*Continued.*

PROTEID (in calories) PER LB. OF BODY-WEIGHT

(Body-weight as taken Jan. 14, 1906)

	Week	B	E	Lq	Lw	M	P	R	T	W	Average
2nd Period	Mar. 28–Apr. 3	1.8	2.6	2.0	2.2	2.2	1.9	1.8	2.7	2.2	2.1
	Apr. 4–10	1.5	2.9	1.7	2.0	1.7	1.5	1.8	2.4	1.8	1.9
	11–17	1.2	--	1.8	1.7	--	--	1.7	1.9	1.9	1.7
	20–26	1.8	--	1.6	1.6	1.7	1.5	1.5	2.3	1.6	1.7
	27–May 3	1.7	2.5	1.7	1.7	2.0	1.7	1.6	2.2	1.8	1.9
	May 4–10	1.6	3.0	1.9	1.9	2.2	1.4	1.8	2.1	1.5	1.9
	11–17	1.6	1.9	1.7	1.9	1.9	1.6	1.9	2.1	2.0	1.9
	18–24	1.5	1.8	1.4	1.7	1.8	1.6	1.6	1.7	2.1	1.7
	25–31	1.1	1.3	1.5	1.6	1.6	1.2	1.7	1.4	1.5	1.4

We observe from Table IV that the men who reduced their proteid the most during the first period were B., P., R., T. and W. Of these the first three only lost weight appreciably, and this was partly ascribable, as we have seen, to reduction in their calories. Careful examination of the figures would seem to show, however, that there is some correlation between reduction of proteid and loss of weight.

During the second period there was a decided reduction of proteid in all cases except that of R., who had already brought his proteid down considerably in the first period. E. reduced his proteid, but not until the last three weeks, when he seemed to try to make up for lost time. E., in fact, was the only man in the club, except possibly M. who (through mere inadvertence) did not follow out the rules of the experiment systematically. It need scarcely be said that this is not stated as censure ; for the very fact of the moderation of E.'s and M.'s mastication added to the value of the final comparisons. Even E.'s sudden reduction in proteid at the end was not maintained two weeks afterward, as was shown by the excretion of nitrogen in June, given in Table VI.

It will be observed that the proteid at the end of the experiment was reduced to a fairly uniform level for all the men. Moreover, the proteid at the end corresponds closely with the results of Professor Chittenden's experiments. This is especially significant in view of the fact that this level was reached unconsciously—for only one of the men, Lq., who used the mechanical diet indicator for the entire club, knew regularly the exact character of each man's food proportions—and without any food prescription as was employed in the experiments of Professor Chittenden. This means that there is a simple way of reducing proteid to the level of "physiological econ-

omy," open to the ordinary man, without the necessity of special knowledge of foods and without the necessity of weighing and measuring food, either by the subject himself or by others.

Aside from the changes in proteid, the proportions of food elements did not vary greatly, the percentages of fat and carbohydrate in the total fuel value remaining very nearly constant. At the close of the experiment it was found that for all of the men the proteid in proportion to the total fuel value was very nearly 10%, having been reduced from about 14%. This reduction in the percentage of proteid was almost entirely offset by the increase in the percentage of fat, which rose from about 30% to about 33% on the average. The percentage of carbohydrate thus remained almost constant. Individual variations were much less than might have been expected. The proteid at the close of the experiment among the different subjects deviated very little from 10%; the proportion of fat varied from 28 to 36%, and the carbohydrate from 51 to 62%.

The results of the experiment may throw some light on the problem of the proper amount of food and food constituents for healthy men eating in a natural manner. For the five men, Lq., Lw., M., R. and W., whose weights showed least tendency to fall and whose average weight at the close of the experiment was 151.4, we find the average total calories were 2620, of which 10.7% was proteid, 33% fat, and 56.3% carbohydrate. The number of calories agrees closely with the estimates (for sedentary persons) of Atwater and Benedict by means of the calorimeter.

TABLE V.

QUANTITIES OF FLESH FOODS CONSUMED (meat, fish, shell-fish, poultry)

(In "portions" of 100 calories each).

	Week	B	E	Lq	Lw	M	P	R	T	W	Average
1st Period	Jan. 17–23	2.7	2.4	1.5	2.6	2.4	2.6	2.3	2.9	2.1	2.4
	24–30	2.3	2.2	1.7	2.0	1.8	2.2	2.1	2.1	2.2	2.1
	31–Feb. 6	1.3	1.9	1.2	2.1	1.6	1.3	2.1	1.9	1.4	1.6
	Feb. 7–13	1.3	1.6	.9	1.7	.6	1.2	1.5	1.8	1.2	1.2
	14–20	.9	2.3	.2	1.1	.7	.9	.9	1.6	.5	.9
	21–27	1.2	2.0	.2	1.6	.6	.7	1.6	2.1	.6	1.1
	28–Mar. 6	.8	2.1	.3	1.5	1.1	1.1	1.6	1.5	.8	1.2
	Mar. 7–13	.7	1.6	.1	2.4	1.0	1.2	1.9	1.6	.9	1.3
	14–20	1.0	1.7	.03	1.9	.9	.7	1.8	1.5	.7	1.1
	21–27	1.0	2.5	.0	2.5	1.7	.7	1.2	2.5	.6	1.4

TABLE V—*Continued.*

QUANTITIES OF FLESH FOODS CONSUMED (meat, fish, shell-fish, poultry)

(In "portions" of 100 calories each).

	Week	B	E	Lq	Lw	M	P	R	T	W	Aver.
2nd Period	Mar. 28–Apr. 3	.9	1.7	.0	1.7	.6	.8	1.1	.8	.1	.8
	Apr. 4–10	.4	1.1	.0	1.0	1.0	.1	.7	.8	.0	.5
	11–17	.1	--	.0	.9	--	--	.8	.7	.0	.4
	20–26	.0	--	.0	.1	.1	.0	.2	.4	.0	.1
	27–May 3	.16	1.2	.0	.6	.8	.2	.9	.5	.0	.5
	May 4–10	.0	.9	.0	.8	.8	.0	.5	.6	.0	.4
	11–17	.0	1.2	.0	.7	.8	.0	.4	.3	.0	.4
	18–24	.0	1.3	.0	.5	.5	.0	.8	.2	.0	.4
	25–31	.0	1.0	.0	1.0	.9	.0	.8	.4	.0	.4

Table V shows that during the first period all except E. and Lw. reduced their consumption of flesh foods considerably. It is noteworthy, as Tables XI–XIII will show, that these two were the men whose improvements in endurance were probably among the least during this period.

During the second period Lq., W., P. and B. virtually abandoned flesh foods entirely, the "portions" consumed daily averaging nearer zero than .1. These men improved greatly in endurance also. On the other hand, E., Lw., M. and R. reduced their flesh foods the least, and their ranking in respect to increased endurance was in general relatively low.

Excretions, Body-Weight, Strength.

The following table of nitrogen excreted in the urine is interesting in connection with the preceding table. It will be seen that the reduction in nitrogen daily excreted corresponds in general to the reduction in proteid consumed.

TABLE VI.

GRAMS OF NITROGEN EXCRETED DAILY.[1]

	B	E	Lq	Lw	M	P	R	T	W
Middle Jan.	10.4	12.7	14.3	14.3	8.7[2]	11.1	14.8	12.2	15.4
First April	6.6	14.7	9.2	11.1	---	6.3	11.6	12.4	9.0
Middle June	6.3	13.1	8.4	---	13.7	6.1	---	8.8	9.4
N. in middle June per kilog. of body-weight	.093	.22	.12	---	.21	.09	---	.12	.13

[1] Each figure is obtained by averaging 2 or 3 consecutive days' specimens.

[2] Jan. 23 and Feb. 10.

This table shows that all the men excepting E. and M. greatly reduced their nitrogen excretion during the experiment, and that at the close (with the two exceptions noted) the men were on about the same nitrogen level as the subjects of Professor Chittenden's experiment, namely, near one-tenth of a gram of nitrogen per kilogram of body-weight.

Through the kindness of Professor Benedict of Wesleyan University, nitrogen analyses were made in December, 1906, six months after the close of the experiment, to discover to what extent the men had adhered to their newly acquired diet after the eating club in which it had been practiced was disbanded. The results were B. 11.0, Lq. 10.5, Lw. 7.9, M. 9.9, P. 6.8, R. 11.5, T. 11.9, W. 8.9. These show that half of the men had reverted to some extent toward their original diets. The men state that the reason for this reversion was the difficulty in selecting food differing greatly in kind and amount from that customarily served at their boarding houses.

The following table shows that the volume of urine daily excreted was greatly reduced during the experiment :

TABLE VII.

VOLUME OF URINE DAILY EXCRETED (in cubic centimeters)

	B	E	Lq	Lw	M	P	R	T	W
Middle Jan.	1425	1160	1130	1391	817[1]	706	1287	1792	1177
First April	630	985	900	1252	....	629	1025	930	797
Middle June	802	1120	822	....	785	480	....	696	970

From this table we see a striking reduction in the volume of urine excreted, with the same two notable exceptions, E. and M. These two, who reduced their excretions least, were the men who were the least assiduous in observing the rules of the experiment.

A careful examination of the feces was made by Professor L. F. Rettger of the Sheffield Scientific School. A summary of his report follows. In it was included a comparative statement for three sets of specimens of two days each, taken in January, March and June, referred to below as series I, II and III. These included data as to the color, odor, quantity, consistency, approximate determination of the number and predominant kinds of bacteria, putrefactive and fermentative properties, and a true microscopic bacterial examination. In brief, to quote from Dr. Rettger's report and letter :

[1] Jan. 23 and Feb. 10.

"The odor was very slight in almost every specimen in the last series, a marked difference between these and series I and II, particularly I. The average weight is less than in both series I and II (av. wt. of series I = 137.3 grams; of II = 164.8 grams, and III = 120.4 grams).

"The figures indicate considerable difference in the putrefactive and fermentative properties of the three series, and the decrease is progressive. In series I the amount of proteid dissolved was much larger than in II and III. . . The specimens [of series III] were more solid generally than in both previous series. I was unable to note any appreciable difference in the microscopic appearance of the last series as compared with the previous, except that in specimen B of the last series a large number of moulds were present. This has little significance, however."

TABLE VIII. FECAL TESTS[1]

	Putrefactive Degree			Fermentative Property		
	Mid. Jan.	End March	Mid. June	Mid. Jan.	End March	Mid. June
B	?	?	8%	+ +	+	+
E	30%	25%	10%	+ +	+	+
M	20%	10%	15%	+	+	+
Lq	60%	?	50%	+ +	+	+
Lw	25%	15%	†	+ +	+	†
P	20%	?	?	+	—	—
R	30%	20%	†	+ +	+	†
T	20%	?	5%	?	+	+
W	30%	10%	5%	+ +	+	+

We here observe that the degree of putrefaction in the last two tests was usually considerably less than its magnitude in the first test. The least change in the feces occurred in the cases of Lq., M. and E., and the greatest changes in P., T. and W. Here again we find some correspondence between the assiduity of the men and the observed physiological changes; for E. and M. were the least and P. and W. the most careful among the experimenters.

A critic has raised the question whether the improvement in the feces indicates lessened absorption of poisons, and whether, if the feces were longer retained, the improvement in their character might not be in consequence of the abstraction from them and absorption

[1] In the table, " + " signifies the presence and " — " the absence of fermentative property; " + + " represents a high degree of fermentative property; " ? " signifies that the putrefactive degree was doubtful, if not absent.

† No specimen.

into the system of a *larger* amount of poisons. The length of time of retention of the feces was not measured in any way. So far as can be guessed from the impressions of the men, it was not lengthened, certainly not greatly, as in the case of Mr. Fletcher.[1] As to the significance of the improvement in feces, Dr. Rettger writes:

"The subject of intestinal putrefaction is one of which very little is as yet known. A retention of feces may have the tendency of lowering the amount of putrefactive products. This is due, I believe, to two things: first, an absorption of such products as indol and mercaptan; and second, an unusual amount of antagonistic action exerted on the evil-producing (putrefactive) bacteria by the ordinary and presumably helpful bacteria. Recent work seems strongly to emphasize the latter point. . . There is nothing to show that a small degree of retention would make a very great difference.

"The absence of appreciable amounts of putrefactive bodies from feces under the ordinary conditions of peristalsis does, beyond a doubt, indicate a lessened production of the products (toxines); the system must be the better off on account of this . . . the interpretation of the facts must be dealt with rather cautiously."

The following table shows the body-weights of the men (after deducting weight of clothing).

TABLE IX.

BODY-WEIGHTS IN POUNDS (without clothing)

	B	E	Lq	Lw	M	P	R	T	W	Average
Jan. 14	148	127	147	153	141	144	179	156	153	149.8
Mar. 28	144	128	147	154	142	136	176	155	151	148
June 16	138	122	146	149	138	131	175	148	149	144

We see that during the first period, the weights, except of P., remained practically stationary, but that in the second period all of the men lost somewhat in weight, though the loss was trifling in most cases. The only substantial losses during the two periods combined were: P. 13 lbs., B. 10 lbs., and T. 8 lbs. Of these it may be said that B. was distinctly over his normal weight at the start.

The distinct correlation between the loss of weight and the reduction in food, and to some extent in proteid, has already been noted. P.'s loss is ascribable largely to overstudy. The general slight reduction in weight of the entire club is probably explained in the same way, for all the men, with possibly two exceptions, distinctly overstrained

[1] See *The A. B.-Z. of our own Nutrition.*

in their college work. Besides the influence of overwork, there was also present the influence of the season,—at least if the common impression is correct that persons usually lose weight with the approach of warm weather.

Gymnasium tests were made at the beginning, middle and end of the experiment. These tests were of two kinds,—tests of strength and tests of endurance. The times of the tests were widely separated, partly because those of endurance were too exhausting to be often repeated, and partly because it was desired to avoid the influence of "practice"; for not only does practice increase strength and endurance, but it also gives the users of the strength-registering apparatus a facility or "knack" in manipulating it which produces a false appearance of improvement.

The dates of the three tests were January 14, a week after the end of the Christmas vacation; March 28, just before the Easter recess; and June 16,[1] just before the summer vacation.

Tests of strength, taken at the beginning, middle and end of the experiment, show the following effects:

TABLE X.

STRENGTH TESTS (in lbs.)

	Date	B	E	Lq	Lw	M	P	R	T	W	Average
R. Grip	Jan. 14	100	135	95	98	145	95	132	120	125	116
	Mch. 28	93	130	110	100	112	102	127	130	117	113
	Jun. 16	91	130	110	92	105	103	125	121	115	110
L. Grip	Jan. 14	94	115	82	112	107	82	131	95	127	105
	Mch. 28	82	115	110	100	105	70	115	106	111	102
	Jun. 16	75	115	97	105	105	70	125	98	110	100
Back Lift	Jan. 14	275	400	280	340	375	250	360	370	365	335
	Mch. 28	260	440	375	380	275	275	390	400	400	355
	Jun. 16	280	300	290	250	330	265	345	330	364	306
Leg Lift	Jan. 14	520	600	370	400	460	320	820	545	635	519
	Mch. 28	515	600	455	450	410	415	865	570	650	548
	Jun. 16	400	545	440	445	400	300	610	520	650	479
Total	Jan. 14	989	1250	827	950	1087	747	1443	1130	1252	1075
	Mch. 28	950	1285	1050	1030	902	862	1497	1206	1278	1118
	Jun. 16	846	1090	937	892	940	738	1205	1069	1239	995

In this table we see that during the first period there was a slight increase in strength (from an average "total" strength of 1076 to 1118), and during the second period a slight fall to 995, which is about 12% from the mid-year's 1118, and about 8% from the original

[1] But May 31 for E., Lw., R. and W., on account of earlier examinations than the others, necessity to leave town, etc.

1076. Thus the strength of the men remained nearly stationary throughout the experiment. The greatest losses were those of B., E. and R., whose records fell respectively from 989 to 846, 1250 to 1090, and 1443 to 1205.

The loss of strength, like the loss of weight, seems most probably explainable by the overstudy of the men. This cause was certainly actively at work, and would apply in the case of all of the club with possibly two exceptions. Overstudy applied conspicuously to B. and R., both of whom not only overworked during the entire period of the experiment, but had, just before coming to the last test, been through the most exhausting and sleep-robbing week of all. There seems, therefore, little reason to ascribe any part of the slight losses of strength to the dietetic experiment itself.

This opinion is confirmed by two facts : One is that the man who was least affected dietetically by the experiment was E., one of the three largest losers of strength, while the men who were most affected dietetically were P. and W., neither of whom lost strength perceptibly, in spite of P.'s severe overwork and loss of weight. The other fact is that in Professor Chittenden's experiment, which dietetically was very similar, the subjects, who were soldiers and athletes and not subject to pressure of work of any kind, showed large gains in strength. From these two facts we may infer that, so far as the diet is concerned, the effect would be to increase rather than to decrease strength.

Changes in Physical Endurance.

It is fortunate that the strength of the men remained so nearly stationary ; for it demonstrates the more clearly that the increase in endurance which will be shown below was an increase in endurance *per se*, and not in any degree due to an increase in strength. Strength and endurance are entirely distinct and should be separately measured. The strength of a muscle is measured by the utmost force which it can exert *once* ; its endurance, by the number of times it can repeat a given exertion *well within its strength.*

After much consideration and consultation it was decided not to place reliance on the ordinary ergographs as a means of measuring endurance.[1] Instead, seven simple gymnastic tests of physical endur-

[1] The reasons, in brief, were (1) because these ergographs are adapted to testing only a few unimportant, and for the most part unused, muscles ; (2) because, in operating these devices, the subjects do not simulate real work, since the mus-

ance were employed, and one of mental endurance. The seven physical tests were :

(1) Rising on the toes as many times as possible.

(2) Deep knee-bending, or squatting as far as possible and rising to the standing posture, repeating as often as possible.

(3) While lying on the back, raising the legs from the floor to a vertical position and lowering them again, repeating to the point of physical exhaustion.

(4) Raising a 5-lb. dumb-bell (with the triceps) in each hand from the shoulder up to the highest point above the head, repeating to the point of physical exhaustion.

(5) Holding the arms from the sides horizontally for as long a time as possible.

(6) Raising a dumb-bell (with the biceps) in one hand from a position in which the arm hangs down, up to the shoulder and lowering it again, repeating the motion to the point of physical exhaustion. This test was taken with four successive dumb-bells of decreasing weight, viz., 50, 25, 10 and 5 lbs. respectively.

(7) Running on the gymnasium track at a speed to suit the subject, to as great a distance as possible.

The mental test consisted of adding specified columns of figures as rapidly as possible, the object being to find out whether the rapidity of performing such work tended to improve during the experiment.

From the wisdom born of experience it may be stated that the physical tests were too numerous and too severe. But after they

---

cles are placed in an awkward and unnatural position in which "no purchase" is felt ; (3) because experience has shown that subjects waste their effort by expending it not only while raising but while lowering the weight, and that this waste during the period of relaxation varies greatly with different subjects ; (4) because a fixed weight is used instead of a weight proportionate to the different strengths of the various subjects. One might as well attempt to test the walking powers of a woman weighing 100 lbs., as compared with those of a man weighing 200 lbs., by compelling the woman to carry a 100-lb. weight so that she might walk with the same weight as the man. Some of these objections have been met in special instruments, such as that of Prof. W. S. Hall of Northwestern University.

After the experiment was half over, and too late to make use of it, the writer devised an ergograph which, it is believed, meets all of the above objections. He was led to do so by the fact that the tests employed were so frightfully exhausting to the men. A description of the new ergograph will be published later. It is to be employed in further tests.

were once adopted in January, it was necessary in subsequent tests to adhere to them, so far at least as always to begin with the same test and follow the same sequence of tests as far as the series was repeated. It is clear that one's ability to succeed in an individual test would depend greatly on what and how many tests had immediately preceded; consequently the only modifications in the January tests which could legitimately be adopted in March consisted in omitting all tests after the first two or three. These first two or three, being taken under the same conditions as before, reflected correctly any change in endurance so far as those particular tests were concerned.

At the final series of tests in June, no omissions from the January program were made; to save time, however, the last two parts of test 6, together with test 7 (which came at the end for all the men), were repeated only up to the point at which they had been carried in January, although the men were able in June to carry them much further, and in many cases did so of their own accord. One man, for instance (W.), who in the run in January was glad to stop at 10 laps, went on in June to 34, running at the same speed until near the end; and this was done after having more than doubled his former records in almost all of the other tests. The unlooked-for increase in endurance made the June tests much more time-consuming than the tests in January and March. Had the men in June taken test 7, and the two last parts of test 6 up to the same fatigue limit as in January, some of them would have had to remain in the gymnasium (supperless) until bed time. One of the men, who in January in the last two parts of test 6 raised the 10-lb. dumb-bells 318 times and the 5-lb. dumb-bells 1,863 times, without doubt could have raised them in June double and probably treble these numbers, but to have done so would have consumed of itself an hour and a half of extra time.

In view, therefore, of the only partial repetition of test 7 and the last two parts of test 6, these records are omitted from Table XI. The first part of test 6 (lifting the 50-lb. dumb-bell) is also omitted, being given separately below.

The following table (XI) shows the results of the three sets of tests in January, March and June.[1] This table will repay careful study.

[1] The order in which the tests were taken was not the same for all of the nine men, owing to the lack of a sufficient number of gymnasium assistants in taking the tests. But care was taken that each man should himself preserve the same order in all three series of tests. Thus, for the March series he took the first two

From it we see that with one exception (E.) all of the men had improved in the March and June tests as compared with the January tests, and the eight men who did improve showed improvement in every test, except Lq., Lw. and T., who showed slight falling off in individual cases.

As inspection will show, some of the increases are remarkable. The recorded increases in the 60 odd cases were, with a few exceptions noted below, all true increases and not due to increased effort to break a previous record. In anticipation of such possible effect of ambition, the men were urged in the January tests to the utmost limit they could or would stand. The original intention had been to work each muscle tested until it was physically unable to repeat the motion, but this was not usually found practicable, except in tests 3, 4 and 6, and in some cases 2. In the other tests the will gave out before the muscles. The March and June tests were so managed that when a man had surpassed his January record he was not allowed to proceed beyond the degree of fatigue which he had reached in the first test. This was usually not a difficult matter, as the fatigue in January had been excessive and the men had no desire to suffer again the painful after-effects. Hence, with the exceptions to be noted, the March and June records not only exceeded those of January, but were accomplished with much less fatigue. The actual improvement was therefore greater than the recorded improvement.

---

or three tests which he had taken in January. This explains why, in the March series, the tests as shown in the tables are not the same for all the men. The order of the January and June tests for the different men is given below. The tests which were taken in March are in italics.

B	*1*	2	3	4	5
E	*1*	2	3	4	5
Lq	*1*	*2*	*4*	3	5
Lw	*1*	*2*	*3*	4	5
M	*1*	*5*	2	3	4
P	*3*	1	5	2	4
R	*1*	2	3	4	5
T	*1*	2	3	4	5
W	*3*	*5*	1	2	4

TABLE XI.

TESTS OF PHYSICAL ENDURANCE.

			B	E	Lq	Lw	M	P	R	T	W
(1)	Rising on Toes	Jan.	300	1007	333[1]	69[1]	127	1482	702	900	1263
		Mar.	400[2]	1265[2]	2620[4]	65[1]	400[7]	----	831[2]	1500[4]	----
		June	500[2]	1061[7]	3000[5]	85[1]	1500[4]	1800[2]	1263[2]	1800[6]	3350[5]
(2)	Deep Knee Bending	Jan.	82	142	70	48	132	208	374	129	404
		Mar.	---	---	191[7]	47[7]	---	---	---	---	---
		June	200[2]	81[7]	202[2]	58[7]	155[2]	230[2]	453[7]	250[4]	508[4]
(3)	Leg Raising	Jan.	25[6]	52[6]	9[3]	22[6]	30[6]	27[6]	50[6]	23[6]	30[6]
		Mar.	---	---	---	33[6]	---	34[6]	---	---	40[6]
		June	33[6]	38[6]	20[6]	35[6]	31[6]	37[6]	103[6]	19[6]	53[6]
(4)	5-lb. Dumb-bell (triceps)	Jan.	75[6]	138[6]	78[6]	38[6]	51[6]	44[6]	100[6]	83[6]	185[6]
		Mar.	---	---	106[6]	---	---	---	---	---	---
		June	127[6]	59[6]	80[6]	51[6]	75[6]	56[6]	104[6]	101[6]	501[6]
			M S	M S	M S	M S	M S	M S	M S	M S	M S
(5)	Holding Arms Horizontal	Jan.	5– 0	1–33	4– 7	3–37	3–30	5–39	2– 5	3–22	11– 0
		Mar.	----	----	----	----	5-49	----	----	----	15–35
		June	9–36[6]	2–56[7]	3–50[7]	3– 0[7]	6– 5	10–1[2]	3–16[7]	3–24[7]	23–45[7]
(6)	25-lb. Dumb-bell (biceps)	Jan.	50[6]	18[6]	16[6]	6[6]	20[6]	11[6]	10[6]	25[6]	54[6]
		June	105[6]	10[6]	26[6]	33[6]	30[6]	29[6]	27[3]	75[3]	108[3]

Criticism of Records of Physical Endurance.

That the fatigue after the March and June tests was in general much less than after the January test was made evident by three substantial proofs. The first was the feelings of the men themselves as recorded in the foot-notes to Table XI. After the March and June tests, every man of the eight who showed improvement felt "not tired," or "less tired than in January test," which is the same as saying "not exhausted"; or else he had gone "to limit" as in January, which means that the muscle itself refused to continue work. The last was usually true of the "leg-raising," "raising 5-lb. dumb-bell (triceps)" and "raising 25-lb. dumb-bell (biceps)". The only tests in which there was the possibility of being mistaken as to the degree of fatigue were the "rising on toes" and "holding arms horizontal." In the former fatigue comes so slowly, and in the latter the pain is so intense that they prove to be tests of will power or "grit" quite as

[1] Cramped.
[2] Not as tired as in January test.
[3] Not to limit.
[4] Not nearly as tired as in January test.
[5] Not tired.
[6] To limit of muscle's capacity.
[7] About same fatigue as in January.

much as of muscle power. In these cases the men had some difficulty in remembering the original degree of fatigue. But the increases were so great and the men were so positive as to their feelings that there remains little room to doubt the substantial correctness of the results. In a few other individual cases, as of Lw. and Lq., whose records in test 1 were sometimes stopped by cramps, there is some room for doubt as to the correctness of the recorded improvement.

The second proof that the fatigue of the men in the June tests was less than that in the January tests was found in the fact that the stiffness and soreness which followed in June were markedly less than in January and of much shorter duration. This was true of all the eight men who showed improvement, except R.

The third proof of less fatigue in June than in January for the eight men is that in June the men finished the ordeal of the endurance tests with more strength left than in January, although, as we saw from Table X, they *began* the two tests with slightly less strength. The fact that they had more strength left after the June test is made evident by the first part of test 6, given below, which in each case came after the endurance tests were nearly or quite finished. This consisted in lifting a 50-lb. dumb-bell. The weight being so great, this was practically a test of strength rather than of endurance. Now all of the eight men who showed improvement in the *endurance* tests of Table XI, showed improvement in this *strength* test also, as the following table shows:

TABLE XII.

LIFTING (by biceps) 50-lb. DUMB-BELL. [1]

	B	E	Lq	Lw	M	P	R	T	W
Jan.	0[3]	1[3]	1[3]	0[3]	1[3]	0[3]	4[3]	2[3]	13[3]
June	1[3]	0[3]	5[3]	8[3]	13[3]	1[3]	10[3]	12[2]	26[3]

But, as we have seen in Table X, the strength tests taken *before* the endurance tests showed a slight falling off in June as compared with January for all but one (Lq.) of these eight men. In other words, in June the men *began* their endurance tests weaker than in January, but *finished* them stronger. The larger residuum of strength

[1] This part of test 6, being one of strength rather than of endurance, was not included in the endurance Table XI. Had it been included it would have increased even more the percentage of improvement shown, for it shows an average increase from 2.4 to 8.4, or 250%.

[2] Not to limit.

[3] To limit of muscle's capacity.

left after the June tests as compared with the January tests indicates that the June tests, in spite of being far more severe, fatigued the men less.

The 50-lb. dumb-bell test resolved the last doubts in my own mind whether, for some of the men, the recorded results might not exaggerate the true improvement. The two men of whose records I should have felt a little doubt were B. and R. Both of them came to the June test after prolonged mental exertion, and their exhaustion at the end was far more evident than that of any of the others. That it was great is clear from their own statements given below, though only R. reported himself as having been about as stiff and sore after the June as after the January tests. But both B. and R., whereas they had less strength (Table X) before the June endurance tests than before the January tests, had more strength left (Table XII) after the June tests than after the January tests. At the close of the January tests they were so exhausted that B. could not raise the 50-lb. dumb-bell at all and R. could raise it only 4 times. Had their exhaustion after the June tests been as great, it seems certain that B. would still have been unable to raise it, and R. would have been unable to raise it more than 4 times; but as it was, B. raised it once and R. 10 times.

The value of such a positive proof that the June tests were more easily endured than those of January was not perceived until the figures were analyzed. Had it occurred to me in time, all the strength tests taken before the endurance tests would have been repeated after them. It is true that the strength tests at the beginning were not of the same muscles as those (the biceps) used in the strength test by dumb-bells at the end, but, as Table X shows, the strengths of different muscles for the most part vary in unison with each other.[1]

It is significant that the only man whose strength, as shown by the above table, was less at the close of the June experiment than at the close of the January experiment was E., who was also the only man whose endurance showed any reduction. The facts, therefore, in his case are not discordant with those already stated; for, as has been stated, E. was the least assiduous in following the experiment. This was

[1] Out of the 108 comparisons of strength (i. e., comparisons for each of nine men in each of four tests for January vs. March, March vs. June, and January vs. June), only 20 are discordant with the general trend as shown by the totals. Thus, for B. the general trend between March and June as shown by the total was downward, and this downward trend is found in all but one of his four tests the discordant case being the "back lift."

often remarked, both to him and to me, by the other members of the club, and it was suggested more than once that I should "nudge" him. But, as I had been desiring a "control," or a subject in which all the conditions except mastication were the same as for the other men, I decided to say nothing. The result was instructive, for E.'s case stood out as exceptional in almost all respects. His reduction in quantity of food (Table III), except for a spurt at the end, was less than of most of the men; his reduction in proteid (Table IV), with the same exception, was the least of all; his reduction in quantity of flesh foods (Table V) was the least of all; his nitrogen in June (Table VI) was one of the highest; his reduction in volume of urine (Table VII) was one of the lowest two; his improvement in the fecal tests (Table VIII) was third to lowest; his loss of strength (Table X) was second greatest; and as to endurance, he was the only one who failed to show improvement.

There was only one other man, M., who was thought, though in a smaller degree, to masticate less carefully than the experiment called for; and for him we find corresponding peculiarities, though in a smaller degree. Thus, his reduction in total daily food (Table III) was less than of most of the men; his reduction in proteid (Table IV) was less than the average reduction; his reduction in flesh foods (Table V) was the third smallest; his June nitrogen was the highest (Table VI); his reduction in quantity of urine (Table VII) was one of the lowest two; his improvement in fecal test (Table VIII) was second to lowest; his loss of strength (Table X) was the third greatest (or fourth, if measured in percentage); and his increase in endurance, though great, was (except in test 1, which is subject to some doubt) less than the average.

The shortcomings of these two men, E. and M., as to mastication were not intentional, but due to carelessness and force of habit, as well as, in the case of M., to the fact that he waited on table and felt naturally more pressed for time. Their experience is valuable in showing that, in a general way, the changes in diet and endurance were proportionate to the thoroughness of mastication and the following of natural appetite.

The men kept diaries in which are recorded their sufferings after the various tests. These show a decided lessening in stiffness and soreness in the later tests, though in the June tests the men had generally done double the amount of work that they had done in January. It would have been a physical impossibility to do as much in January as was easily accomplished in June in tests 3, 4 and 6; and granted

that it had been even possible in January to goad the men to do as much in tests 1, 2 and 5 as they did without urging in June, they must certainly have been ill.

The following are statements from the men themselves :

Personal Impressions at end of Experiment.

(B) I was *very sore* [after the June test, Saturday, June 16, 1906]. However, I think the soreness was not so severe or lasting as it was after the January test. The muscles of my right arm were swollen considerably and I was unable to straighten it for two or three days. But the swelling was not so severe as it was after the earlier test and the arm was much more usable. The muscles of the thighs were the sorest ; they were nearly worn out. During Sunday, Monday and Tuesday after the test I had difficulty in walking ; and going down stairs was quite a difficult and severe undertaking. Wednesday morning the soreness had not left, though it had decreased considerably. I took a considerable tramp that day, and by night I could scarcely feel the soreness at all. By Thursday I had practically regained my normal endurance ; walked six or seven miles that day. The calf-muscles too were quite sore, but much less so than after the January test. There was another particular distinction. After the earlier test the calf-muscles were hard and knotted for several days ; but this time, while they were sore, they were almost normally soft. Saturday evening when I went to bed they were quite hard, but Sunday morning they were normal and practically remained so. . . .

I have no doubt that in my case there was great increase in endurance, though I think that I lost in amount of energy that I could exert at any given moment. This loss is due perhaps to two things ; (1) I took, on the whole, less exercise than during the time preceding the January test ; (2) I had been working quite hard for three months steadily, while the January test followed a three weeks' vacation during which I did little or nothing. As to increase of endurance there can be no doubt. For example, in the deep knee-bending, I began to get tired at 50 and had no idea of going above 100. When I reached this I set my goal at 125, then 150, 160, and was able to reach 200 before I was exhausted. In January, after I was tired I was not able to go on very long before I became completely exhausted. This shows increased endurance. I had the same experience in the other hard physical tests. In case of the run, I was sure I could not go more than three laps after my first lap ;

but I made 11 or 12 and could have gone several more. Considering everything, I have no doubt that I was able to hang on much longer, after I began to get tired, than in January.

I am at a loss to ascribe the increased endurance to anything else than to the diet. My way of living otherwise continued about the same after the January test as it was before. . . . Personally I am convinced that the increased endurance must be due to diet and manner of eating ; all other factors that I can think of are unfavorable rather than favorable to more endurance. I am convinced to the extent that I shall certainly continue "Fletcherizing" and using a low-proteid diet.

(E) All effects of [June] test disappeared entirely within four days. [Effects of January test lasted six days.]

(Lq) The stiffness and soreness had entirely disappeared in four days. It was not nearly so severe as the test in January. After I was through in January I could hardly go down the stairs of the Gymnasium, and three days after the test going up and down stairs was accompanied with a great deal of pain. . . . I was stupid mentally for a whole week the first time, but in the last test I passed that stage in a couple of days. . . . Had it not been for the late hours and long stretches of work, I should have been able to make a better comparison with conditions in January, though as it was results show improvement. . . .

I cannot say as to the help mentally I have derived, for I have, always gone to my limit and I would be unwilling to make any positive statement. As for the physical, I know there is an improvement there, for my stomach, which was never so very strong, has been greatly helped.

(Lw) There was no stiffness or soreness felt in the triceps or the stomach muscles as the result of the last tests. The thigh muscles were a little stiff on the second day only—about such stiffness as one might expect from a long walk. The calves of my legs began to stiffen on Friday [June 15, 1906, the day after the test] and continued to do so on Saturday, after which the stiffening began to lessen, and was scarcely felt on Monday. The biceps of my right arm gave me the most trouble. These were sore on Friday A. M. and continued to increase in soreness till Sunday evening, feeling worst, however, Sunday A. M. When I arose Monday A. M. all the soreness and stiffness had disappeared. A peculiarity about the latter which impressed me was the fact that although my arm was very sore it did not seem to be very stiff. After the tests in January I could not

straighten my arm, but I could after the last tests in spite of the extreme soreness. I had entirely recovered by Monday from the tests. At no time after the tests did I feel any pain in proceeding up and down stairs, and if I remember rightly I couldn't say the same in January; neither did I feel particularly uncomfortable at any time. After the half-mile run and the lifting tests which I took later, I felt no soreness or stiffness afterward. . . .

I think the credit must be given to the diet experiment. I have worked harder from January to June than ever before, and have taken less exercise. As my mental work was so different from that previous, I cannot form an estimate of any increase or decrease in efficiency, but as I have said before, I always rested up more quickly. During the spring I have not felt that "all gone feeling" which usually has appeared in the past. The diet which we have had has relieved me of the sour stomach after meals, and I have felt better and worked harder on less exercise than ever before. . . . After a moderate amount of exercise, I have felt no such stiffness as used to come.

(M) The stiffness and soreness were entirely worn off in two days. I did not feel it nearly as much as I did last January. In fact, I did not exert myself to the utmost this last time because I had several examinations to take a day or two later.

My general impression is that the experiment was an all around benefit to me. I fully believe that during the tests, they reflected the true state of the case in showing my efficiency in June compared with that in January. I believe that there was a decided improvement in efficiency and could ascribe it partly to my exercise and the other part to the new manner of eating. I believe, however, that my exercise played a very small part because I think what I gained in exercise I lost in sickness [mumps].

My exercise this year was practically the same as years preceding. After April 1st I had very little exercise, on account of the mumps. This left me in a weak condition over a month. I had lots of work to make up and studied harder from April to June than any other period of my course. My exercise was neglected these three months and I studied almost constantly every day and until 12 at night.

My experience has shown me that I was at my best in mind and body when I ate meat four times a week. I have tried both more and less and found the above to be the medium. I also found that I could do more when I had the largest meal at noon. The greatest benefit of the experiment to me personally is that last year I broke down in the spring term and this spring I kept up my work and health in a much better condition.

(P) I went into the second test with some trepidation, knowing that I had lost considerable weight the preceding ten weeks. . . . Physically, I cannot say that I felt stronger before the second test than before the first; nor did I feel weaker. As the test developed, however, I soon saw that my endurance, both mental and physical, had increased.

Generally speaking, the soreness was less extensive, less trying or acute, and (I think) shorter-lived than in January. . . . It seems to me, as I finished the test much fresher than in January, a clear gain in efficiency is proven. The test seemed certainly to make a true report. . .

I can ascribe gain in endurance to nothing but the diet and thorough mastication. Every other factor in the situation was against this gain—exercise, of which I took certainly no more than usual and in the latter weeks much less; work, of which I had had a long, hard pull as against the three weeks' rest preceding the January test; sleep, much decreased for most of May and June. You stated last December that you wished every factor to be *in favor* of the first test and *against* the second. This condition has been true in high degree for my case. . . Whatever the efficacy of the two tests in proving the superiority of low proteid and thorough mastication for the other members of the club, I feel convinced that they prove that superiority with considerable force in my own case.

I have tried meat and chicken a number of times in the last two weeks, partly from curiosity and partly from necessity. But in every case anticipation has been pleasanter than realization, and my low-proteid tendencies bid fair to remain for some time to come. I may say that I had no opinion on the diet question when the experiment started, but am now a hearty low-proteid exponent. . .

I went into the test with considerable foreboding as to my endurance showing; for I have worked now without a break for twenty-two weeks at hard mental labor, the last two weeks being especially confining and involving large losses of sleep and exercise. I may say that I have been unusually well for six or eight weeks, and bowels have been running with greater ease and constancy than for several years. . . The endurance-tests, showing a good increase in every test, consequently came as a complete surprise; and my self-confidence, largely absent at the start, returned in increasing measure as the test went on. . .

Thursday, June 21 [5 days after test]. Played golf this morning and afternoon (9 holes each time) with perfect ease, no difficulty

with walking or driving the ball. Soreness wholly gone at present writing, no touch of it noticeable anywhere.

(R) The outcome of the last endurance test was about the same as the one held January 14, 1906. I was fearfully sore for about one week, reaching the climax at the middle of the week.

(T) Throughout the test I passed from one event to another with much shorter periods between than I did in January. With the exception of lying on my back and raising my feet, I at no time approached as near exhaustion as I did in January. In January, in rising on my toes and in the deep knee-bending, I continued till I fell to the floor. I was not exhausted at the close [of the June test], but marked papers for 2½ hours before going to bed. Sunday I scarcely felt any the worse, though my muscles felt a little queer when I poked my finger into them. Monday my leg muscles were a little stiff after a period of rest, but not painful in the least. My right shoulder was a trifle lame, due wholly I think to hitting it once in a while in the last test with the 10 and 5-lb. weights. My right arm at the elbow was decidedly lame and would not admit of being completely straightened, though it was undoubtedly better than in January. By Tuesday all the other stiffness had practically left me except the right elbow, which was, however, better. By Thursday I was unable to detect any soreness whatever in any part.

(W) May 31. After the test I felt fairly tired and ready to quit —however, not nearly so exhausted as before in January. Could walk down stairs with more confidence and could raise my supper to my mouth much more easily than after the first test. . . . The results certainly far surpassed any expectation I had, especially as in the morning I did not feel quite as spry and active as usual, due to a little unusual over-exertion the previous day.

June 1. Sore in thighs and biceps, also felt my abdominal muscles.

June 2. Expected to be much worse on this the second day, as in January, but not so. About same as yesterday. Later in the day could run up-stairs two steps at a time as I could yesterday—a thing undreamed of in January for over a week after the test.

June 3. Felt pretty well today, much improved over yesterday, still felt my thighs in walking down hill or down stairs, but not nearly as bad as yesterday.

June 4. Feel my thighs only very little, other muscles not felt at all. Rode a bicycle 5½ miles ; did not feel it.

June 5. Seem to be all well, haven't noticed a soreness all day.

The following table expresses the percentage of improvement in the records of Table XI.

TABLE XIII.

IMPROVEMENT IN PHYSICAL ENDURANCE IN PERCENTAGES.

		B	E	Lq	Lw	M	P	R	T	W
(1)	Jan.–Mar.	33+	26±	686–	–5±	215±	---	18+	66+	---
	Jan.–June	66+	5±	800–	23±	1081±	21+	79+	100+	165+
(2)	Jan.–Mar.	---	---	172±	–2±	---	---	---	---	---
	Jan.–June	144+	–43±	188+	21±	17+	10+	21±	94+	26+
(3)	Jan.–Mar.	---	---	---	50	---	26	---	---	33
	Jan.–June	32	–27	122–	59	3	37	106	–17	77
(4)	Jan.–Mar.	---	---	36	---	---	---	---	---	---
	Jan.–June	69	–57	2	34	47	27	4	22	170
(5)	Jan.–Mar.	---	---	---	---	66±	---	---	---	42±
	Jan.–June	92–	89±	–7±	–17±	74±	77+	56±	1±	115±
(6)	Jan.–June	110	–44	62	450	50	163	170	200	100+
Av.	Jan.–Mar.	33+	26±	298±	14±	140±	26	18+	66+	37±
	Jan.–June	85±	–13±	194±	95±	212±	56+	73±	66±	109±

In the preceding table most of the figures are succeeded by a "+", which signifies that the true improvement was greater than the figures indicate. Thus, the first entry in Table XIII, "33+", means that B.'s improvement between January and March in test (1) (rising on toes) was *more* than 33%. Similarly, "686–" for Lq. in same test means that improvement was *less* than 686%. Again, "215±" for M.'s same test signifies that his improvement in this test may have been greater *or* less than 215%. Finally, when any figure is not followed by a sign, as for instance, B.'s (3) (leg raising), the meaning is that the figure given is, humanly speaking, correct. This accuracy applies only to those tests in which the muscles were worked till they were physically unable to repeat the movement. The reasons for the various suffixes may be found by studying the foot-notes of Table XI.[1]

[1] For instance the "+" after 33 for B.'s (1) is explained by the fact (as indicated in the foot-note to Table XI) that after his March test he was not as fatigued as after his January test, although he had improved upon his January record by 33%. The only cases in which the explanation of the suffixes will not be found from the foot-notes to Table XI are the following: E.'s (1), 26±, in which case the "–" is inserted owing to the fact that E. had come to the March test after the refreshment of a nap; and M.'s (1), 1081±, in which case the "–" is inserted owing to the fact that this high figure is inconsistent with the other results of the test, it being thought that M. may have been mistaken in his

The table shows enormous differences in the figures even of the same man for the same period. Thus, the June improvement of W. reads 165+, 26+, 77, 170, 115±, 100+. Such wide differences between the improvements in different tests seem puzzling at first, but they are explained, partly if not wholly, by two reasons. The first is the obvious one that many of the figures are not exact records, but understatements, and naturally their margin within the truth will vary widely. Thus, the records for deep knee-bending (2) for W. show merely that the improvement is *over* 26%; the true figure may well be 100%, which would be more consistent with the other figures. But the deep knee-bending test had been found in January very painful and inconvenient in its after-effects, and there was therefore less inclination in the June tests to approach closely to the limit in this particular test.

The other reason is that in some tests a larger fraction of the total strength of the muscle tested was called into play than in others. Thus, "leg raising" requires a very large fraction of the strength of the abdominal muscles, while "rising on toes" requires only a small fraction of the strength of the calf muscles. This may explain why, in general, the improvement in the test of the calf muscles seemed so much greater than in that of the abdominal muscles. This explanation is, however, purely hypothetical. It would be interesting to find out experimentally how much an improvement in the endurance of a muscle shows itself when it is exerted in different degrees, say to 75%, 50% and 25% of its strength-capacity.[1]

Bearing in mind these two possible reasons for the variations in the figures, and also the fact that there must have been more or less actual differences in the improvement of different muscles, we need not be surprised at the disparities which the table shows.

If we omit the cases in which the records are at all doubtful (with suffix ±) or exaggerated (suffix −), we have left the following table for the eight men who showed improvement :

---

remembrance of his January test. The "−" has been inserted whenever there was the slightest ground of any kind for thinking the figures might be overstatements. With these figures weeded out, the remaining ones certainly understate the actual improvement.

[1] The original object of using the graded dumb-bells, 50-lb., 25-lb., 10-lb., and 5-lb., for testing the biceps, was to throw light on this problem; but for reasons previously stated, these tests were not fully carried out.

TABLE XIV.

PERCENTAGE OF IMPROVEMENT (exact or understated) OF EIGHT MEN.

		B	Lq	Lw	M	P	R	T	W
(1)	Jan.–Mar.	33+	---	---	---	---	18+	66+	---
	Jan.–June	66+	---	---	---	21+	79+	100+	165+
(2)	Jan.–Mar.	---	---	---	---	---	---	---	---
	Jan.–June	144+	188+	---	17+	10+	---	94+	26+
(3)	Jan.–Mar.	---	---	50	---	26	---	---	33
	Jan.–June	32	---	59	3	37	106	–17	77
(4)	Jan.–Mar.	---	36	---	---	---	---	---	---
	Jan.–June	69	2	34	47	27	4	22	170
(5)	Jan.–Mar.	---	---	---	---	---	---	---	---
	Jan.–June	---	---	---	---	77+	---	---	---
(6)	Jan.–June	110	62	450	50	163	170	200	100+
Av.	Jan.–Mar.	33+	36	50	---	26	18+	66+	33
	Jan.–June	84+	84+	181	29+	56+	89+	80+	107+

The figures of Table XIV show an undoubted increase in endurance, both for the first half and more especially for the whole period of the experiment.

But, for an accurate presentation, we may carry our criticism one stage further. The figures given hitherto represent a conglomerate sort of endurance, made up of endurance of different muscles subject to different degrees of strain. As pointed out before, the calf muscles were called upon for only a small fraction of their strength-capacity, whereas the abdominal muscles were called upon for a very large fraction. Moreover, the fraction must have varied somewhat in different tests, according to the variation in strength and weight. An ideal test would be one in which the same fraction of strength was used. [1]

Fortunately, such an exact test is afforded by the 25-lb. dumb-bell. It followed immediately after the 50-lb. dumb-bell had been raised until the biceps was unable to repeat the motion. At the moment the 50-lb. test ended, the 25-lb. test began. At this moment the strength of the biceps was just at or barely below the fifty lbs. required to raise the heavier dumb-bell. In other words, in raising the 25-lb. dumb-bell the muscle needed *just fifty per cent.* of its strength at the time the test began. The use of the 25-lb. dumb-bell gradually reduced this strength from 50 to 25 lbs. The test was there-

[1] It is on this principle that the new ergograph, before referred to, is constructed.

fore perfectly uniform for all the men; it showed how many contractions were necessary in each case to bring down the strength of the biceps from 50 to 25; it showed how much the muscle could endure before being robbed, by fatigue, of half its strength. Thus at the beginning the strength is 50 lbs.; after the first contraction it is, say, 49; after the second, 48, etc. But the contractions continue until the strength sinks below 25 lbs. The loss of strength may be said to measure fatigue. The *slowness* of this loss may be said to measure endurance and is well indicated by the number of contractions necessary to tire a muscle from a strength of 50 lbs. to a strength of 25 lbs.

Four exceptions, however, need to be noted. Three men, B., Lw. and P., were unable in January to raise the 50-lb. dumb-bell at all (see Table XII). Consequently their January test with the 25-lb. dumb-bell did not begin at 50% of the strength, but at a higher fraction. This explains their high apparent improvement. Thus, Lw. is credited with an improvement of 450%, because in January he could raise the 25-lb. dumb-bell only 6 times, and in June, 33 times. But the 33 contractions in June began at just 50% of the strength of the muscle, owing to its previous exhaustion to the 50-lb. level by the 50-lb. dumb-bell, whereas the six contractions in January began at a higher level; for at that time the biceps could not raise the 50-lb. dumb-bell at all. Its strength was at that time less than 50 lbs., say 40 lbs., in which case the lifting of the 25-lb. dumb-bell required not 50% but 62½% of its strength. To compare a 50% test of June with a 62½% test in January gives a record of improvement which is not one of pure endurance, but which includes the element of increased strength. This is "endurance" in the crude sense in which we may say a man has more endurance for carrying trunks than a boy; but for a comparison of pure endurance, the boy should be given smaller trunks to handle than the man.

The fourth case is E., to whom the reverse reasoning applies. In June when he reached test 6, he was unable to raise the 50-lb. dumb-bell at all, though in January he had raised it once. Hence, while the 25-lb. dumb-bell was a 50% test in January, it was a more severe one in June, and the −44% which records his falling off does not represent a pure loss in endurance, but partly also a loss of strength. To reckon *pure* endurance we need to bring −44 up toward zero.

Making the four omissions just mentioned, we may use the remaining records from the last line of Table XIII, as a barometer of *pure* endurance.

We therefore have three methods of estimating the increase of endurance between January and June. These may be put together in the following table :

TABLE XV.

PERCENTAGE OF INCREASE OF ENDURANCE, JANUARY TO JUNE.

BY THREE METHODS.

	B	E	Lq	Lw	M	P	R	T	W
Average 6 tests	85±	-13±	194±	95±	212±	56+	73±	66±	109±
Omitting doubtful cases "+"	84+	---	84+	181	29+	56+	89+	80+	107+
"Pure" endurance of biceps	---	---	62	---	50	---	170	200	100+

The first line of this table tells us the average of the recorded improvement in endurance shown for each man. But as each such average is made up from the figures of Table XIII, some of which, as indicated in that table, are possibly too high, some doubt necessarily attaches to it, though practically the only real cases of doubt are Lq. and M. The average of these averages is 101% for the entire club, and is probably within the truth ; for most of the individual figures which go to make up this result are understatements, not overstatements.

The second line shows the average improvement in tests in which there is no doubt that the figure is at least not too high, though it may be too low. The average of these is 89%, and is therefore certainly too low an estimate of the average improvement for the eight men who improved at all.

The third line shows the increase of *pure* endurance (that is, endurance considered apart from strength) for the five men for whom the figures were available. The average of these is 116%.

We are quite safe in saying therefore that the average improvement of the eight men who improved was 90%. As to the degree of retrogression of E., it is difficult to say, though it is believed that the figures exaggerate it. This is certainly true of the 25-lb. dumb-bell test, for reasons given. My own impression, and E.'s also, is that he actually gained in endurance from the dietetic experiment, but that his gain was not enough to offset the loss occasioned by (1) the hard term's work, which, as in the case of the other men, was a decided handicap, and (2) the omission of his customary exercises, which must have

been a greater handicap in his case than in any other of the men; for he had been accustomed for six years to heavy gymnasium training, but during the year of the experiment this training was given up, largely because of the difficulty in finding time for it. If this interpretation is correct, we may liken the experiment to nine men trying to swim against a current. The eight who exerted themselves the most succeeded in forging ahead; the one who tried the least drifted backward, though the effect of the swimming (dieting) was to propel him forward. Whether or not E. was actually propelled forward by diet must remain a matter of conjecture or inference; but that the other eight men gained is an established fact.

Changes in Mental Endurance.

The mental test consisted in adding a specified number of figures. The following tables show the time during which the addition was performed and the number of errors committed:

TABLE XVI.

TIME OF PERFORMING A UNIFORM AMOUNT OF ADDITION.

		B		E		Lq		Lw		M		P		R		T		W		Average	
		M	S	M	S	M	S	M	S	M	S	M	S	M	S	M	S	M	S	M	S
Time of adding	Jan. 14	5	40	4	49	6	15	4	54	6	46	3	1	7	6	6	41	4	6	5	29
	Mar. 28	5	16	4	27	4	35	4	15	5	47	2	43	6	32	7	18	4	34	5	3
	[1] June 16	4	50	5	9	4	40	4	23	5	50	2	58	7	3	6	5	4	7	5	0

This shows that during the first period seven had improved and two had fallen off, and on an average there had been a decrease from 5m. 29s. to 5m. 3s., an average improvement of 26s. W. showed an increase in time of adding, although he would naturally have been expected to improve on account of having taken up clerical work involving adding.

During the second period there was an average improvement of only 3s.; three retrogressed 15s. to 42s., three retrogressed 3s. to 8s., and three improved 26s. to 73s. The fact that the men held their own in the June adding test is probably indicative of actual improvement, for they were fatigued mentally by examinations, etc., on the day when they entered the June test. During the entire experiment there was an average improvement of 29s.; seven had improved and two had retrogressed (1s. and 20s.)

[1] E., Lw., R. and W. taken on May 31.

The following table shows that the number of errors committed was remarkably constant for most of the men and for the average :

TABLE XVII.

NUMBER OF ERRORS OF ADDITION.

		B	E	Lq	Lw	M	P	R	T	W	Average
Errors	Jan. 14	10	8	12	4	1	1	2	1	1	4.4
	Mar. 28	16	5	8	4	3	0	3	2	1	4.6
	June 16	8	13	5	4	2	1	6	0	2	4.5

The adding test was too short to be of great value. In future tests a larger number of figures will be employed, and a different method. After the specified amount of adding has been done, it will be at once repeated on another equivalent set of examples. The excess of time required for the second set over that required for the first may be called the "fatigue time," and this fatigue time, taken as a percentage of the total time of adding, may be used as a criterion of endurance—the less it is, the greater the endurance, and *vice versa*. This plan was developed too late to be put into operation at the beginning of the experiment. It was, however, employed in the March and June tests, and confirmed the conclusion reached above, that there was little difference between the mental endurance in March and June. Five of the men showed a less "fatigue time" in June than in March, and four a greater.

The following statements of the men themselves will show that their feelings as to working power were in harmony with the conclusion that it had improved :

Subjective Impressions as to Mental Working Power.

B. (March) "Not decreased at any rate, seems to have increased."
(June) I did more work during the latter part of year than I ever did before in an equal period of time. But, I had the work to do and compelled myself to do it. However, I was mentally tired at the close of the year, particularly so at the time of the test, for it came after the siege of exams for which I did my own work besides a couple of days of hard tutoring.

This much, at any rate, is positive : There was no decrease of mental power resulting from the experiment. I was no more tired at the close of last year than I was at the close of the year before. After a week's rest I felt quite normal and then did considerable mental work all summer.

E. (March) "Working power improved. Can concentrate attention for a longer time."

(June) "I accomplished a greater amount of mental work than in previous years during the corresponding period of the college year. I do not think that my feeling of fitness for it was any greater, however, and I cannot say that my experience of fatigue after the work was any less. I learned to eat slower than had been my custom during previous years. Though not subject to indigestion, I experienced less stomach disorders during the period of the experiment."

Lq. (March) "I have put in more long hours during this term than any previous term, consequently have had a good deal less sleep. I do not know that I can work any better, except that I can work a longer period at one time without feeling so tired from it."

(June) "Of course a great deal of the extra work was outside work which was an extra tax. I, however, did a great deal more work on papers that I prepared than I ever had before. . . . Although I spent longer hours than before I did not feel the effect of the work so much as before."

Lw. (March) "Have been working harder during the past four months and have taken less exercise than at any other equal period during past 2½ years. The character of the work has been so different that I am unable to say whether there is any increase in working power, but I find that I rest up very quickly after becoming tired (mentally).

"When March tests were taken I did not feel as 'fit' for test on that particular day as at time of January tests. Had been working hard and had been under nervous strain, which undoubtedly affected the tests."

(June) "I have worked harder from January to June than ever before, and have taken less exercise. As my mental work was so different from that previous, I cannot form an estimate of any increase or decrease in efficiency, but as I have said before, I always rested up more quickly."

M. (March) "I think on the whole a slight improvement."

(June) "Do not notice any change."

P. (March) "I have never worked so steadily, or with so little necessity to exercise the will to work, as in the central six weeks of the test. The work I was doing was chiefly research in the Library, poring for three or four hours at a time over old records—not labor of the most interesting kind."

(June) My work from March to June was of a more confining sort than ever before in the spring. From March to the Easter recess I was occupied with library research; from Easter on I was engaged on an essay and the marking of some 260 Social Conditions theses. On these last I spent considerably more time and effort than in the preceding year. Thus my work was harder and more time-filling than usual. I had much less out-door exercise than in previous springs, and missed that relaxation from effort which all prior springtimes have bred. Yet there was no feeling of overwork, or even of work as a burden, till the first of June. From then on I did feel tired, and examination time found me with a mind very difficult to keep in harness. Undoubtedly I had over-pushed myself, but did not realize it till June. . .

R. (March) Felt an increase in efficiency.

(June) My power for mental work was greater between the March and June tests than between the January and March tests and the latter was greater than before the experiment began in January. I can state without hesitation that my mental working power increased in consequence of "Fletcherizing."

T. (March) Felt that he had at least held his own, but "surprised to find that every one of my tests (physical) had improved." For the first test came after the rest and recreation of the winter holidays when he "was in splendid condition. Since then I have had to work extremely hard with little regular exercise and rarely in bed before about midnight." Surprised also that the mental test showed no improvement, probably because "the confusion around me was considerably greater than in the first test." Can do his ordinary mental work faster than before, though not sure that he can work longer.

(June) "I consider I did more work last year during the period of the experiment than any other year. During the whole nine months of the college year I was practically working up to my limit of endurance. I did not grow sleepy as early evenings as previous years and my attention was not as easily distracted from my work as previous years.

"The lack of improvement in the second mental test may have been due largely to the fact that I was mentally fagged out after the examinations and was feeling the need of my holidays."

W. (March) No definite impression either of gain or loss.
(June) "On the whole I felt quite as workish as ever I did in the spring months and did not feel the hot days as much of a drag as usual."

As to illness, in the course of the experiment there were the usual winter colds, though apparently these were less common than before. One man had grip for a few days, another the mumps, and several had constipation. In general, the men expressed themselves as better than usual and in some cases they were very enthusiastic. None of the ailments suffered by the men were ascribable to the test itself, unless it be a case of what appeared to be slight rheumatic sensations of T., who had always been a heavy meat-eater, and who during the experiment introduced at first much acid fruit. That the acid in conjunction with the high proteid might occasion such symptoms is at least consistent with some of the numerous theories of rheumatism. On avoiding very acid fruits he soon lost all these symptoms.

The following extract from the diary of B. is, I think, typical of the facts in this respect to general health: "Have now, March 23, slight sore throat. In regard to colds, I have been troubled less this year than at any time for years; but this fact may be due to great change in climate, Nebraska to Connecticut. Have usually had colds more or less all winter; therefore my freedom has been indeed remarkable. From September last till the beginning of the experiment I experienced frequent attacks of indigestion, 'heart-burn.' Have been almost free from that, though two or three times I had the same experience after eating bananas."

Summary.

The phenomena observed during the experiment may be summarized as a slight reduction of total food consumed, a large reduction of the proteid element, especially for flesh foods, a lessened excretion of nitrogen, a reduction in the odor, putrefaction, fermentation and quantity of the feces, a slight loss of weight, a slight loss of strength, an enormous increase of physical endurance, a slight increase in mental quickness. These phenomena varied somewhat with different individuals, the variations corresponding in general to the varying degree in which the men adhered to the rules of the experiment.

That we are correct in ascribing the results, especially in endurance, to dietetic causes alone, cannot reasonably be doubted when it is considered that no other factors of known significance were allowed

to aid in this result. On the contrary, so far as the operation of other factors was concerned, these must have worked against rather than for the results achieved. Exercise was in no case indulged in to a greater extent than had previously been the custom, and in most cases it was less. The men were warned not to take up exercise, except so far as they had been accustomed to before the experiment began, and if they varied their exercise at all, to lessen rather than increase it. They were very conscientious on this point, as on others, —so much so that some of them at first gave up exercising until they began to feel "logy." This over-zeal was corrected; but in no case have I reason to think that the exercise taken was more, or more systematic, than previously. M. was probably the most systematic in taking exercise. His statement on this point, as previously given, the reader may care to review.

The men did not practice on the endurance tests between times. This was expressly forbidden, and the men were too trustworthy to admit of a doubt on this point. The tests themselves, needless to say, were too far apart to have given any chance for repetition to give "knack," and were too severe to count as beneficial exercise.

Nor were the men more regular in their hours of retiring or other habits. On the contrary, they were rather more reckless in burning the midnight oil. It developed that, with their increased freedom from fatigue, they indulged more freely than ever their propensity to work in the lines of their respective ambitions. At first they felt justified in doing this, as it accorded with their instructions not to remove any handicaps to their chance of improving their endurance, but to increase rather than decrease such handicaps. But this liberty became license, and I was forced to remonstrate with the men for their late hours and overstudy, which tended to rob them of their surplus endurance almost as fast as it accrued. Long before the experiment was finished the men had given every appearance of improved working power, but I was not at all sure that they would have any of it left to show in the final test, because of their tendency to use it up in work. Had the extent of their working proclivities been realized in advance, it is doubtful if the experiment would have been undertaken at all. It should be stated that all except M. were graduate students, and almost all of them, in addition to their university work, were earning their own way.

The advance of warm weather must have tended, had not their diet counteracted it, to tire the men, if, at least, we may trust common impressions as to "spring lassitude."

Again, the conditions immediately preceding the March and June tests, as compared with those preceding the January test, were such as to give the advantage to the January test. The latter came soon after the Christmas holidays, when the men, as they themselves stated, felt refreshed and at their best, whereas the March test came just before the Easter recess, after a hard term's work, and the June tests came after a like period of hard work,—in some cases, as of B. and R., immediately after exhausting examinations.

Finally, the tests themselves were serious drains on vitality. Each required a period of from several days to two weeks for recuperation, and each robbed the men temporarily of several pounds of weight. The cookless diet experiment for six days also cost something to those who took part in it.

In addition to the tests mentioned in this report was one on January 23 of leg-raising, deep knee-bending and arm-stretching, taken after a night from which two hours of sleep were purposely cut off. After consideration, it was decided not to repeat this test as being too fatiguing. It therefore has been omitted from this report; but it added one more burden for the men.

When, therefore, we observe the known handicaps,—the over-study, the strain of the tests, the advance of warm weather, the fact that the first test came after rest and the other tests after work, and when we are unable to find any other cause than diet—such as exercise, regularity of bed-time or other habits—we are forced to conclude that the only causes which produced the endurance were dietetic.

Possibly some persons may be disposed to find a convenient escape from this conclusion by ascribing the improvement to suggestion. Under this theory, the men improved because they expected to. It is quite true that there may be more force in autosuggestion than most of us realize. But, fortunately, for the present case we scarcely need to argue the point; for as a matter of fact it was not true that all of the men expected to improve. This was certainly not true before the March test. In fact, the men were about equally divided in their predictions as to the outcome, and used to have animated discussions. Yet, both the confident and the skeptic faction improved in endurance in the March test; and so far as I am acquainted with their prognostications and have noted their improvement, there was little if any correlation between those prognostications and their improvement.

It is of course still *possible* that some unobserved element has crept

into the case, to which, and not to the diet, the improvement in endurance was due; but in view of all the facts recited, this is extremely *improbable.* What slight doubt remains should be resolved by further studies. I earnestly hope that other and more careful studies may be made by more competent investigators than I.

We conclude that the improvement in endurance was exclusively due to dietetic causes. The only dietetic causes at work were (1) thorough mastication, (2) implicit obedience to appetite, (3) (during the second half of the experiment) when appetite did not clearly determine the choice, the voluntary selection of the non-flesh and low-proteid foods, and (4) an ample variety of good foods, well cooked.

So far as cooking is concerned, this cause, as has been said, entered unintentionally. But there is no evidence that it was a prime factor in the experiment, while there is some evidence to the contrary. Thus, E., who especially remarked the culinary virtues of the cook and who missed her services more than any one else during the brief period of her absence, was the one member of the club who failed to improve in endurance.

If we allow ourselves to speculate as to the changes in the character of diet which were produced by thorough mastication, we may draw an inference from the fact that the carnivorous animals are fast-eaters, whereas the grain-eating animals are slow-eaters. It would seem, therefore, when man changes his habits from fast eating to slow eating he naturally changes his food from the food of a fast-eating to that of a slow-eating animal. The question, therefore, which is the natural food for man, may possibly be associated with the question, which of the two methods of eating is natural to man. Was the slow eating of the nine men an artificial and unnatural practice, as would be indicated from the fact that the majority of men eat far faster? Or, are the ordinary habits of man in respect to the manner of fast eating themselves unnatural? I have not attempted to gather the facts necessary to solve this problem, but it certainly constitutes an interesting one for the physiologist and anthropologist. The few facts upon which I have chanced to fall would seem to indicate that man is naturally a slow eater, and that the hurry-habit to which most of us are prone is a consequence of the artificial high-pressure to which modern civilization has subjected us. Certain it is that the conditions which give rise to quick-lunch counters and to the short stops of trains for refreshments, were produced, not in order to meet any natural propensity to eat fast, but on the contrary, in the

interest of the more rapid transaction of business, with which meal-times are regarded as an interference.

We may therefore at least conclude that whatever the speed of eating which is natural to the human animal, his actual speed under civilized conditions is greater than natural. It is noteworthy also that children are very deliberate in eating their cookies. It is only after they are reproved for keeping their elders waiting that they begin to imitate the latter and bolt their food. Dr. Higgins [1] and Dr. Hasse [2] have pointed out also some physiological considerations, based on the anatomy of the human throat compared with the throats of the carnivores and of "poltophagic" animals, which would indicate that man, to a large extent at least, is naturally a slow-eating animal. Dr. Henry Campbell [3] has also given some evidence, based on a study of the primitive tribes, to show that chewing is more thorough among uncivilized races, and that the hurry habit to which we are accustomed is largely promoted by the use of prepared and 'mushy" foods,—which, in fact, appear to have been devised expressly for the purpose of being quickly swallowed.

The evidence, however, on the natural food-habits of man is as yet very meager, and it is only provisionally that we may consider the thorough mastication advocated by Mr. Fletcher as "natural." With this reservation we may say that the experiment here described may be called an experiment in natural eating, or an effort to restore a blunted or lost food-instinct, so that it may serve as a safe guide to the proper quantities and kinds of foods. If it be asked in what way this natural eating tended to improve endurance, whether it was because of the finer sub-division of food through mastication; the increased "insalivation"; the increased flow of "appetite juice"; the better adaptation of foods to the particular needs of the individual and the moment; the lessened quantity of food; the lessened proteid; or the lessened amount of flesh foods, no satisfactory answer can be given, although, as the previous discussion shows, there is more or less evidence on some of these points. There are certainly some very fascinating problems for the physiologist to solve in regard to fatigue as related to diet. Are the "fatigue poisons" due, for instance, chiefly to the combustion of proteid in excess of the physiological

[1] See *Humaniculture*, N. Y. Stokes, 1904.

[2] See Archiv für Anatomie (Waldeyer's) 1905, p. 321.

[3] "Observations on Mastication," London *Lancet*, July 11, 18, 25 and Aug. 8, 1903. Reprinted in Horace Fletcher's *The A. B.-Z. of Our Own Nutrition*, Stokes, 1903. See pp. 126–135.

needs, as the theory of Chittenden would explain them? Or, are they largely due to the ingestion of these poisons with flesh foods, as the vegetarians and Dr. Haig have maintained? Or, do both explanations have a share?

The results of the experiment demonstrated so great an increase of endurance as to seem at first incredible. It certainly was a surprise, both to the men and to me. But statistics which I have been collecting during the last two years have prepared me to find great differences and changes in endurance. The special result of the present experiment is to show that diet is an important factor in producing such alterations. The fact that endurance, even among persons free from disease, is one of the most variable of human faculties—far more variable than strength, for instance—is evident to any one who has made even a superficial examination. Some persons are tired by climbing a flight of stairs, whereas the Swiss guides, throughout the summer season, day after day spend the entire time in climbing the Matterhorn and other peaks; some persons are "winded" by running a block for a street car, whereas a Chinese coolie will run for hours on end; in mental work, some persons are unable to apply themselves more than an hour at a time, whereas others, like Humboldt, can work almost continuously through eighteen hours of the day. Among statistics gathered independently of the present experiment, I have found measurable differences between persons far greater than the change of endurance of the eight students which we have seen.[1] Among some 50 tests of different persons holding their arms horizontally, many were found whose arms actually dropped against their will inside of ten minutes, whereas several were able to hold them up over an hour, and one man held them 3 hours and 20 minutes, or a round 200 minutes, and then dropped them voluntarily. Similarly with deep knee-bending, some persons were found physically unable to rise again from the stooping posture after accomplishing less than 500 bendings, whereas several succeeded in stooping 1,000 times, and in one case, 2,400. Again, in leg-raising, the legs positively refused to rise to the vertical in some cases before 40 times were reached, whereas in two cases this motion was performed 1,000 times or over. On the new ergograph previously referred to, among the 16 preliminary tests there was a range in endurance between different persons from 18 to 145 and in the same person at different times from 29 to 110.

[1] For an account of some of these statistics see "The influence of flesh-eating on endurance." *Yale Medical Journal*, March, 1907.

It is, to say the least, remarkable that hitherto so little effort has been directed toward discovering the factors which explain such differences in endurance. That exercise is one of the most and perhaps the most important factor has alone been recognized. A correspondent assures me that by means of moderate *regular* exercise he succeeded in increasing his endurance between 100 and 200% in three weeks as measured by leg-raising and "dipping." The influence of diet has always been regarded as small or negligible, and the opinion has been almost universal, until recently, that a diet rich in proteid promotes endurance. Even among those whose researches have led them to the opposite conclusion, there is very little conception of the extent to which diet is correlated with endurance. Such a person, a medical friend of the writer, stated, when the present experiment was planned, that he did not think the dietetic factor strong enough compared with others to produce any marked effect. We have all heard, of course, of the enthusiastic reports of vegetarians as to their increased endurance, but these we have discounted as exaggerations. The result of the present experiment, however, would seem to indicate that one's improvement in endurance is usually not less, but greater, than he himself is aware of. Probably it is also true that we may lose a large fraction of our working power before we are distinctly conscious of the fact.

While the results of the present experiment lean toward "vegetarianism," they are only incidentally related to that propaganda. Meat was by no means excluded; on the contrary, the subjects were urged to eat it if their appetite distinctly preferred it to other foods.

The sudden and complete exclusion of meat is not always desirable, unless more skill and knowledge in food matters are employed than most persons possess. On the contrary, disaster has repeatedly, overtaken many who have made this attempt. Pawlow has shown that meat is one of the most and perhaps the most "peptogenic" of foods. Whether the stimulus it gives to the stomach is natural, or in the nature of an improper goad or whip, certain it is that stomachs which are accustomed to this daily whip have failed, for a time at least, to act when it was withdrawn.

Nor is it necessary that meat should be permanently abjured, even when it ceases to become a daily necessity. The safer course, at least, is to indulge the craving whenever one is "meat hungry," even if, as in many cases, this be not oftener than once in several months. The rule of selection employed in the experiment was merely to *give the benefit of the doubt* to the non-flesh food; but even a *slight* preference for flesh foods was to be followed.

Under flesh foods are included all meat- and "stock-" soups. It has been shown that although these extracts of meat contain a large amount of nitrogen, it is not in the form of proteid which can be utilized, but only of waste nitrogen which must be excreted. Apparently the sole virtue of such soups is that they supply the "peptogenic" stimulus above referred to.

The experiment will be seen to harmonize with and supplement the experiment of Professor Chittenden, on which it was founded; but the objects of the two experiments were quite different. Professor Chittenden's was aimed to ascertain the physiological requirements as to proteid, and did not touch upon the question of endurance. Moreover, Professor Chittenden, in order the better to measure the proteid and nitrogen, artificially reduced the quantities ingested, whereas in the present experiment, test was made of Mr. Fletcher's claim, that thorough mastication leads naturally to the adoption of the physiological amount of proteid. This we found to be true, especially after the introduction, at the middle of the test, of the suggestion that when appetite was in doubt, the lower proteid foods should be selected. But the tendency was quite marked during the first period also, and might have been expected to lead to the same results without the introduction of even the suggestion of voluntary choice, had the experiment been long enough. This was the experience of others, notably Mr. Fletcher himself, whose case, in fact, first called Professor Chittenden's attention to the possible virtues of low proteid.

The practical value of the experiment consists in the fact that any layman can apply it, with or without a knowledge of food values, though with more advantage if he possesses than if he lacks such knowledge.

If the dietetic rules of the present experiment are followed, no self-denial as to foods is required. It is, however, absolutely necessary that there should be *self-control* enough to break up the habit of hurried eating to which modern civilization has brought us, habituating us, as it does, to eat against time.

Experience indicates that appetite does not lead to a diet fixed in amount or constituents, but moves in undulating waves or cycles. The men who took part in the experiment were encouraged, after any of the symptoms which seemed to be associated with high proteid (such as heaviness, sleepiness, stiffness or soreness after exercise, or catching cold), to cut down on their proteid and substitute fat to restrain the gastric juice. This advice was intended to make

application of the theories of Folin[1] that we usually carry a reservoir of proteid, enough to supply our needs for body-building for a fortnight. If this reservoir is exhausted, proteid starvation occurs and the body feeds on itself; if it is filled too far it overflows and causes the evils of excessive proteid. If this theory is correct, the art of eating may consist largely in maintaining a golden mean such that the proteid reservoir is neither empty nor overflowing, or at any rate, not overflowing much. Many persons fear to reduce their proteid to the Chittenden minimum for fear of proteid starvation; but the experience of those who have tried it would seem to show that this fear is groundless, *provided* no violence is done to natural appetite. This may be trusted, so it would appear, to raise a warning in the form of "nitrogen hunger" before the danger point is reached.

---

[1] "A Theory of Protein Metabolism." *American Journal of Physiology*, March, 1905.